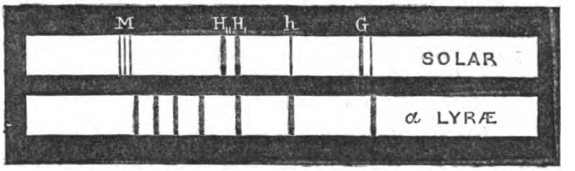
the same plate through the other half of the slit on the following morning. A few lines only of the solar spectrum are given in the diagram. This photograph takes up the spectrum where the eye-



observations fail. The first strong line coincident with the hydrogen line near G had been seen and compared with hydrogen. For the other lines we are indebted to photography. Photographic spectra have been taken of the Moon at different times, and also during the recent eclipse; also of Venus, Sirius, and some other stars. The writer prefers to reserve any discussion of the results until a larger series of observations have been made.

Since the writer communicated his results to the Royal Society last December, a note by Dr. Draper has appeared in the scientific journals, in which he says:—

"Since the spring of 1872 I have been making photographs of the spectra of the stars, planets, and moon, and particularly, among the stars, of *a* Lyræ and *a* Aquilæ, with my 28-inch reflector and 12-inch refractor. In the photograph of *a* Lyræ, bands or broad lines are visible in the violet and ultra-violet region unlike any thing in the solar spectrum. The research is difficult and consumes time, because long exposures are necessary to impress the sensitive plate, and the atmosphere is rarely in the best condition. The image of a star or planet must be kept motionless for from ten to twenty minutes; and hence the driving-clock of the telescope is severely taxed.

"During last summer I obtained good results, and in October took photographs of the spectrum of Venus which show a large number of lines. I am now studying these pictures, and have submitted them to the inspection of several of my scientific friends, among others Professors Barker, Langley, Morton, and Silliman. There seems to be in the case of Venus a weakening of the spectrum toward H and above that line, of the same character as that I have photographically observed to take place in the spectrum of the Sun near sunset" *.

WILLIAM HUGGINS.

The Determination of the Solar Parallax.—Part I.

THERE is no problem in Astronomy of more importance, certainly none which at present possesses so deep and general an interest, as

* 'American Journal of Science and Arts,' vol. xiii. January 1877.

Digitized by Google

the determination of the Solar Parallax. In proof of this we have only to look to the universal activity and interest excited by the last Transit of Venus, not alone among professional and amateur astronomers, but amongst the educated public generally; and the reason is not far to seek. The problem is of a nature which appeals strongly to every thoughtful mind, the problem how and in what way we can apply our yard-measure to the measurement of the celestial spaces. Apart entirely from the great importance of the problem in gravitational astronomy, the question interests the lover of the exact (the first qualification of the scientific mind) from its very difficulty, interests every lover of science from his love of truth for truth's sake, and appeals even to the commonest mind in the everyday question, "What is the Sun's distance?" It is no wonder, then, that the Transit-of-Venus Expeditions of 1874 left amid expressions of universal good will, and that the public demanded and the press supplied the fullest accounts of their doings. Already questions are beginning to be asked, "What is the result? and when will it be published?" The object of the present article is to show the difficulties which surround the question, describe what has been already discovered as to what is good and what is bad in that which has been done, and to consider what are the means at our disposal for the settlement of this important determination in the future.

Now, what are the broad and general features of every direct mode of measuring the distance of a celestial body? The answer may be combined with a definition of parallax, viz., It is only necessary to measure the difference of the direction of the body when viewed simultaneously by two observers whose relative positions are known. Simple as seems this operation, the practical difficulties are enormous, and tax to the utmost the last refinements of theory and observation. When a surveyor plans a triangulation he is not satisfied if any of his triangles have an angle so small as 30°; and in great geodetic operations, where the highest accuracy is aimed at, triangles where any angle is less than 45° are not admitted. In the problem before us the critical angle is always less than 18″. The present state of astronomy demands that the solar parallax shall be determined with an accuracy of not less than $\frac{1}{1000}$ of its amount. This supposes that the combined errors of the observations of both observers should be considerably less than $\frac{1}{50}$″ of arc. Measures of high accuracy in the neighbourhood of the Sun are attended with considerable difficulty, on account not only of the effect of the Sun's heat on the various parts of the instrument employed, but also of the atmospheric currents created by heat, which tend to destroy steady definition. Thus it happens that the combined measures of all the astronomers of Europe have not yet definitively measured the Sun's diameter with an accuracy of $\frac{1}{10}$″ of arc; how utterly hopeless is it then to look for any solution of the problem by the direct method!

Two courses remain—either to seek some method which affords a larger angle to measure, or to select a method which permits a mode

Digitized by Google

of observation affording a higher precision; and if both these qualifications can be combined, so much the better. Now there are many relations to the solar parallax which are well established. Such, for example, are:—

1. The mass of the Earth. That is to say, there is a well-determined relation between the mass of the Earth and the Sun's parallax, expressed in the equation

$$\pi = 608''{\cdot}79\sqrt[3]{m},$$

when π is the Sun's mean horizontal parallax, and m the mass of the Earth. If, then, the Earth's mass can be independently determined, the solar parallax becomes known. Or, again, we know with very considerable precision the parallax of the Moon by direct determination. The proportion of this parallax to that of the Sun is an important term in the lunar theory; and the constant of this term (the parallactic inequality) is a known function of the solar parallax. Hence, the constant of the parallactic inequality being independently determined, the solar parallax becomes known.

2. The relation between the time of revolution of a planet and its mean distance from the Sun is known by Kepler's third law. The time of revolution and the form of the orbit are known for all the planets; hence, for any instant, the relation between the planet's distance from the Earth and the Earth's mean distance from the Sun is known. To determine the solar parallax it is, therefore, only necessary to measure at some favourable opportunity the parallax of any planet, and to multiply the parallax so found by the number which expresses the relation of the distance of the planet from the Earth to the Earth's mean distance from the Sun.

3. When Jupiter is in opposition he is nearer the Earth by the diameter of the Earth's orbit than when in conjunction; hence, since light occupies a very sensible time to travel, eclipses of his satellites will seem to occur too soon in the first case and too late in the latter, the difference between the extremes of acceleration and retardation being the time occupied by light in crossing the Earth's orbit. This time is about $16\frac{1}{4}$ minutes for the mean diameter of the earth's orbit; hence, if the velocity of light can be independently determined, the diameter of the Earth's orbit becomes known.

The determination, by employing the velocity of light, is also arrived at in another way. The constant of aberration, or the maximum apparent change of a star's true place due to the motion of the observer, depends on the relative velocity of the motion of the observer in space compared with the velocity of light. The angular velocity of the observer is perfectly known: hence, if his linear velocity is known, his radius of motion is known. Thus, if the constant of aberration and the velocity of light are independently determined, the radius of motion (that is the sun's distance) will be found.

Here, then, are methods representing three distinctive types, which we shall classify as:—

Digitized by Google

1. The Gravitational or theoretical method.
2. The Instrumental or practical method.
3. The Physical method.

In a most able and comprehensive paper communicated to the Paris Academy of Sciences, and published in their 'Comptes Rendus' for 1872, July 22, M. Le Verrier has pronounced strongly in favour of the first or the gravitational method. His reasoning is briefly as follows:—

"The attraction of the Earth produces sensible inequalities in the motions of Venus and Mars. By determining the amplitude of these by means of observations, it is possible to conclude directly from them the mass of our planet. To increase the precision of the result we may unite the consideration of the periodic inequalities with that of the secular inequalities. The elements of the orbits of Venus and Mars undergo variations called secular, which increase from year to year, from century to century, and at last acquire very large values, which renders them very suitable for resolving the question with which we are now occupied."

The variations of the excentricities, of the perihelia, of the inclinations, and of the nodes of *Venus* and *Mars*, may all be made to contribute to the result. The most important of these variations is that of the perehelion of *Mars*. The Earth's attraction increases by 50″ in a century the heliocentric position of Mars at perihelion. When Mars is in opposition this change subtends at the Earth an angle of 185″.

On 1672, October 1, the star ψ Aquarii was occulted by Mars; and the occultation was observed by Richer at Cayenne, by Picard near Beaufort, and by Romer at Paris. The separate comparisons differ only 0″·5, 0″·8, 0″·3 respectively; and Bradley has left numerous observations of this star. The increase in two centuries of the geocentric longitude, corresponding to the distance of the planet Mars from the Earth on 1672, October 1, is 294″. Now an error of $\frac{1}{300}$ in the measurement of this variation will only produce an error of one third of that amount, or $\frac{1}{900}$, in the resulting solar parallax. Hence M. Le Verrier concludes that (attributing an error not greater than 1″ to the determination of the variation) the time has arrived when the solar parallax can be determined with a probable error not exceeding $\frac{1}{900}$ of its amount, or the concluded value for the parallax will be exact to nearly $\pm 0''{\cdot}01$. The value of the parallax so deduced, M. Le Verrier finds

$$8''{\cdot}866.$$

Similarly, M. Le Verrier finds from the latitude of Venus, determined by the transits of Venus in 1761 and 1769, combined with the latitude determined by meridian observations of the present day,

$$8''{\cdot}853.$$

From the discussion of the meridional observations of Venus in an interval of one hundred and six years, he finds

$$8''{\cdot}859.$$

Digitized by Google

The marvellous accordance of these results is certainly strongly corroborative of M. Le Verrier's conclusions, that the solar parallax may be deduced from the theories of the planets with an accuracy of $0''{\cdot}01$, an accuracy which he is without doubt perfectly justified in not according to any instrumental determination yet executed.

Nor does M. Le Verrier forget the possible influence of the minor planets; for he points out with much force that if their total mass has any such influence, it must be greater on Mars than Venus; and the coincidence of the results from Mars and Venus shows that there is no sensible effect. At the same time it is impossible to forget the extraordinary intricacy of the processes through which these results have been evolved, and the liability to some systematic source of error overlooked by theory—such, for example, as some neglected term producing a long inequality, which may be mixed up with the secular variation. On this point, M. Le Verrier's own words seem, without further commentary, to be conclusive:—

"But it is necessary here to remark that, as the angular errors introduced by the perturbations in the positions of the planets Mercury, Venus, the Earth, and Mars go on increasing, in the course of time an epoch must arrive when it will no longer be possible to put all these quantities in accord with each other without introducing new forces. This is even now certainly the case with respect to the regions situated between Mercury and the Sun. Thus there is a considerable quantity of matter which has hitherto escaped our regular investigations."

A fair inference to be drawn from this remarkable paper may be put thus. The Sun's mean horizontal parallax is $8''{\cdot}86$; and this value is exact within $0''{\cdot}01$, subject only to the correction due to possible errors of theory, errors which our present knowledge does not permit us to foresee.

M. Le Verrier concludes by pointing out that "the determination of the solar parallax by means of the Transits of Venus" (and we shall take this as understood to include all instrumental methods), "still retains all its interest, but conditionally on its being made with exceptional precision, so that the astronomer may be able to answer for it with an accuracy exceeding $\frac{1}{100}$ of a second of arc."

This conclusion must be indorsed by every astronomer who attentively follows M. Le Verrier's reasoning. Or, to put it otherwise, to bring theory to the test of practice the accuracy of the determination of the parallax should not be less than $\frac{1}{1000}$th part of the whole. Therefore, in the after-consideration of any method, the first test to be applied to it is, *Will it yield this accuracy? If not, it must be considered useless.*

The other gravitational method we have mentioned is the constant of the Moon's parallactic inequality. At first sight no method is more promising. The constant is approximately $125''$, so that the error of its determination is diminished about fourteen times in the resulting solar parallax; and the observations available are most numerous.

Digitized by Google

Unfortunately the determination is inextricably mixed up with the determination of the Moon's diameter—a diameter increased by irradiation, and therefore different for every telescope, and perhaps for every observer. But this is not all. One half of the observations for parallactic inequality are made when the Sun is above the horizon, and a great portion of the other half during twilight; whilst those on which the Moon's diameter depend are made at midnight, when the irradiation is a totally different quantity compared with what it is in daylight or during twilight.

Newcomb has attempted to determine the correction of the diameter by the errors of R.A., derived by comparing Hansen's Tables with observations made by daylight and at night; but he confesses that the result is so mixed up with the correction of the coefficient of the variation (and, he might have added, with the observer's personality and the telescope employed) that it cannot be relied upon. Indeed it is only necessary to quote the results for this constant, arrived at by eminent theorists from nearly the same series of observations, to be satisfied that the method cannot be employed for our purpose, unless, indeed, some new and better method of determining the Moon's place can be discovered :

Airy, from Greenwich observations of the Moon from 1811 to 1851	124″·37
from alt-azimuth observations alone.....	125″·50
and he finally adopts.....	124″·70
Hansen, from the observations at Greenwich, coupled with the observations at Dorpat, derives..	126″·46
Stone, from 2075 Greenwich observations, derives ..	125″·36
Newcomb, from Washington observations alone	125″·46

One cannot look with confidence upon any method which permits a discordance of more than one per cent. in the discussion of the same observations by different astronomers; indeed any method which does not contain within itself the means of eliminating systematic errors must always be subject to this objection.

In the case before us the systematic error is in the diameter of the Moon. If it had so happened that the parallactic inequality could have been measured at its maximum and minimum by observations of the same limb, then the determinations of the same observer with the same telescope, and under the same circumstances of illumination, would have been free from systematic error, and the probable error of the result might have been deduced with considerable approximation. But as it is, the error of the diameter enters into the result; and that diameter not being capable of determination under the same circumstances of illumination as those in which the observations for the parallactic inequality are made, the judgment of the theorist must step in and assign some more or less hypothetical grounds for the adoption of a particular diameter; and upon this assumption will turn the whole of the quantity of which we are in

Digitized by Google

search. Granted the highest skill, the most consummate acumen in such matters, it is still unsound and unsatisfactory that any determination (and especially one of such critical importance and capable of rigid measurement) should depend even in the slightest degree on a question of judgment. This proposition must commend itself to every exact mind, without illustration, and, coupled with the remarks already made, points to the entire rejection of the Moon's parallactic inequality as a method for determining our problem. The same train of reasoning also leads to a second rule or test which it is necessary to apply in the selection of a final method for determining the Solar Parallax. It is this :—

No method can be admitted which is in the remotest degree liable to systematic error; and every method in which the errors are not self-correcting is liable to such systematic error. That is to say, the observations themselves must afford the material for the determination and elimination of all errors to which it can be shown the method is liable.

A third rule or test results almost as a corollary :—

The method must rest on a simple system of rigid accuracy both as to observation and reduction, so that every theorist or computer must, from the same series of observations, arrive at precisely the same result.

D. GILL.

The Nebular Hypothesis, and the Obliquity of the Axes of Planets to their Orbits.

ACCORDING to the Nebular Hypothesis of the origin of the solar system, the planets were masses left behind by the contraction of the solar nucleus, as it cooled, under the influence of the mutual gravitation of its parts. These planetary masses then began themselves to contract under a like influence. As, however, they were originally parts of the great solar nebula, there appears to be no reason why the axes about which they rotated should not be nearly parallel to the axis of rotation of the Sun—that is to say, nearly perpendicular to the planes of their orbits.

The nebular hypothesis seems therefore to contain no explanation of the great diversity which the telescope shows us is to be found in the inclinations of planets to their orbits. The inquiry, of which the present paper is an account, is an endeavour to suggest a cause which will fill up this hiatus in the theory.

The precession of the equinoxes of a planet is a very slow retrogression of the intersection of the planet's equator with the plane of its orbit along the latter plane; and it is caused by the attraction of the Sun and the planet's satellites on the equatorial protuberance. The velocity of this retrogression increases when the equatorial protuberance or the obliquity increases, or when the rate of rotation

Digitized by Google

Mr. Christie read a paper by Prof. Zenger, "On a new form of Solar Eyepiece," in which the Sun's intense light was enfeebled by reflexion at the bounding surface between two kinds of glass (in optical contact), the second glass being very slightly denser than the first, so that very little light would be reflected.

The following is a list of the papers presented:—

Prof. Zenger. A new Solar Eyepiece.

Sen. Arcimis. Lunar Eclipse observed at Cadiz, 1877, Feb. 27.

Adm. Shadwell. Table for facilitating the determination of Mean Equinoctial Time to the end of the present Century.

Gen. Meigs. A method of making a Pendulum swing in an approximately Cycloidal Arc.

J. M. Wilson and G. M. Seabroke. Second Catalogue of Micrometrical Measures of Double Stars made at the Temple Observatory.

P. Secchi. On the alleged fall of an Aërolite at Supino, Italy.

Astronomer Royal. Extracts from Letters received relating to the search for the supposed Intra-Mercurial Planet.

A. W. Downing. A determination of the semidiameter of Venus at mean distance of Earth from Sun.

M. de Boë. On a method of destroying Vibrations in a Mercurial Reflector.

R. A. Proctor. The approaching Opposition of Mars.

E. Neison. On a general method of treating the Lunar Theory.

Prof. Pritchard. Elements of the Orbit of Comet III., 1876 (Swift's).

D. Gill. On the Opposition of the Minor Planet Melpomene, as a means of determining the Solar Parallax.

Lord Lindsay. On the Spectra of the two Comets *b* and *c*, 1877.

R. A. Proctor. Note on the Distribution of the Fixed Stars.

The Determination of the Solar Parallax.—Part II.

LEAVING the instrumental method for after discussion, we come now to the physical method by determining the velocity of light.

What are the systematic errors to which it is liable? The first that suggests itself strikes at the root of the whole method, viz. Is it certain that the velocity of light in the celestial spaces is precisely identical with (or bears an exactly known relation to) the velocity of a ray which, having travelled a certain space in air, undergoes reflexion and returns? This is a question for the physicist, and a question which probably demands a practical as well as a theoretical answer. In other words, it would probably be equally unsafe to rely on a value of the solar parallax derived exclusively from a determination resting on the velocity of light, as it would be to rely on the completeness of a mathematical theory of light in which a well

Digitized by Google

executed physical determination of the parallax differed by much more than the calculated probable error from an unobjectionable instrumental determination of the same constant. In either of the two physical methods any error in the determination of the velocity of light enters of course directly into the result, whether the error arises in the terrestrial determination or in the adopted relation between the velocity in air and the velocity in space.

We shall not enter here into this question, because in the present paper we desire only to deal with the astronomical features of the problem. The velocity of light in space we shall for the present, then, assume to be, or to be capable of being, accurately determined, and proceed to consider the astronomical part of the problem.

The phenomena of Jupiter's satellites are affected with two errors, the errors of observation and the errors of the Tables; and both of these may affect the result with a systematic error. If, however, observations in long series, by the same observer with the same instrument, are secured, and the same phenomena are grouped together in the formation of the final equations, it is possible that systematic error in the observations may be eliminated. To arrive at a precision of 1-1000th part it would be necessary to observe the change of time in the eclipse of a satellite with an accuracy $=\frac{16\frac{1}{4}\text{ minutes}}{1000}=0^{s}\cdot975$, and this on the supposition that the observations are made when Jupiter is in opposition and also when in conjunction. It is of course impossible to observe the phenomena of the satellites very near conjunction, and therefore the full amount of the double factor can never be obtained in practice. Considering this, and having regard to the fact that this must include the combined errors of the observations near opposition and those near conjunction, as well as some error in the light-velocity determination, it is absolutely essential that the combined errors of the Tables and observations either at opposition or conjunction shall not exceed $0^{s}\cdot3$ or $0^{s}\cdot4$. This accuracy is certainly far greater than any single observation of so gradual a phenomenon as the eclipse of a satellite can afford; but it is possibly not greater than might be derived from many hundred observations of the eclipses of the first satellite, provided these observations were made with one telescope by the same individual, and with all necessary precautions to eliminate systematic error.

In the present state of the Tables of Jupiter's satellites, however, no result of any value can be looked for from this method. The solution of the problem requires, in the first place, a complete and sustained series of observations, such as that described by the Astronomer Royal in vol. xxxii. of the 'Monthly Notices.' The satisfactory completion of such a series in a reasonable time demands a climate and a latitude more favourable than any afforded by this country; indeed the establishment of an enthusiastic observer in some rainless district, or in any good climate near the equator, could do more in two or three years to solve this important problem than

Digitized by Google

the utmost devotion of a lifetime in a climate and latitude so unfavourable as our own.

Such a series of observations being secured, the investigator who may devote himself to the problem of the correction of the Tables must of necessity consider the time occupied by light in crossing the Earth's orbit as one of his unknown quantities; and there will be time enough to discuss the matter more fully when new Tables of the satellites founded on such observations appear. Then the accuracy with which observations can be made will be demonstrated; in the meanwhile the argument for the necessity of having better Tables of Jupiter's satellites does not require to be strengthened by the argument that they are required in order to give a determination of the solar parallax. The importance and necessity of such a piece of work rests on its own basis, and should be undertaken apart from all other considerations. When finished, if found to possess the required accuracy, the result can be woven into the web of our problem.

We now come to the second physical method, which employs the constant of aberration. Of all the constants in astronomy which are known by direct measurement without an augmented factor, there is probably none which is so rigidly determined, so entirely trustworthy, as Struve's determination of the constant of aberration for seven stars which pass near the zenith of Pulkowa.

The observations were made by means of his well-known Prime-vertical transit. The symmetry and elegance of the method, the consummate skill of the observer, the perfection and stability of the instrument, and the thoroughness of the discussion, alike combine to place it among the most certain determinations of astronomy. The following Table exhibits the results arrived at by Struve from these observations :—

Name of Star.	R.A.	Dec. N.	Maximum of aberration in Dec.	Resulting constant of aberration.	Probable error.
	h m	° ″	″	″	″
β Cassiopeæ	0 0	58 16	17·9	20·422	±0·035
δ „	1 15	59 24	16·7	·456	·046
υ Ursæ Majoris...	9 40	59 47	15·4	·457	·030
ι Draconis.....	15 20	59 31	20·0	·479	·022
δ „	18 21	58 43	20·3	·504	·032
ο „	18 49	59 12	20·3	·404	·023
P. xix 371	19 53	58 28	20·1	·395	·033

From which he concludes that the constant of aberration is

$$20''{\cdot}445 \pm 0{\cdot}0111.$$

In the paper of M. Le Verrier before mentioned the author makes the remark :—"It would be very interesting, at the time when the constant determined by M. Struve plays so special a part,

Digitized by Google

to be possessed of the grounds of the opinion of that eminent astronomer on the degree of accuracy he is sure of having attained." In reply, M. O. Struve submits a most interesting paper, entering into the details of all possible sources of systematic error which might have affected his father's determination; and, having regard to these and all subsequent observations made with the same instrument, he sums up with the conclusion that $20''{\cdot}445$, as given by his father and universally accepted by astronomers, may still be considered the most probable value of the constant of aberration, and its probable error may be considered $\pm 0''{\cdot}017$.

If the constant of aberration is alike for all stars, it is impossible not to agree with this conclusion; but the elder Struve, in coming to it, adds:—"Qu'il faut supposer dans les sept étoiles, la même constante de l'aberration, c'est-à-dire la même vitesse de la lumière."

At the Meeting of the Academy on 1872, Oct. 14, M. Villarceau made a very interesting communication, "On the Constant of Aberration and the Velocity of Light considered in relation to the absolute movement of translation of the Solar System." In this paper the author draws attention to the fact that, in the ordinary astronomical theory of aberration, no account is taken of the Sun's motion of translation through space. That is to say, in computing the direction and velocity of the observer, only the Earth's motion in its orbit and the Earth's motion of rotation are taken into account. He then proceeds to explain, on the principles of the undulatory theory of light, the effect on the constant of aberration of the motion of the Sun in space, and, investigating the expression, finds that, neglecting some small terms of the second order, the largest of which does not amount to $0''{\cdot}002$, the usual expression of the correction for aberration holds good with the addition of a small constant. Thus the normal constant of aberration being A, the constant for any star is $A + A \times \alpha$, in which α must be computed for each star.

M. Villarceau gives the expression for α, and finds that it depends on the angle which the star's direction makes with the direction of the Sun's translation in space. Hence, if the constant of aberration is the same for a number of stars, one of two things must be true,—(1) either the velocity of the motion of the Sun's translation is insensible compared with the velocity of light, or (2) the direction of this movement makes the same angle with all the stars. The author then remarks that if we assume with Struve that the small differences in his results are due to errors of observation, we must conclude either that the absolute velocity of the Sun's translation in space is insensible as compared with the velocity of light, or that the direction of the Earth's movement is nearly parallel with the Earth's axis *. It is by no means necessary to accept this alternative, because the

* The reader must here remember that the stars employed by Struve necessarily culminated near the zenith of Pulkowa; they therefore all have nearly the same declination, and hence make nearly the same angle with a point in the direction of the Earth's axis.

Digitized by Google

geometrical conditions of the problem demand for its satisfactory solution the determination of the constant of aberration of at least four stars, and these not all situated in the same great or the same small circle of the sphere.

M. Villarceau suggests, as the only satisfactory solution of the problem, the establishment of a Prime-vertical transit-instrument in the Southern Hemisphere. The instrument and method of observation should be as similar as possible to that adopted at Pulkowa, and the determination should be made with the same accuracy. Such a determination would, when combined with the Pulkowa determination, afford the data for determining the true value of the aberration, as well as the direction and velocity of the absolute translation of the solar system in space, if that velocity prove to be sensible compared with the velocity of light.

M. Villarceau shows that the best possible result would be obtained by similar determinations of the aberrations of two circles of zenith-stars, one at a latitude of 35° 16′ North, the other in latitude 35° 16′ South.

It is to be noted that M. Villarceau's remarks do not apply merely to our motion of translation with respect to the other stars which constitute our nebula or cluster, the Milky Way. With regard to these the labours of Struve, Argelander, and Peters show that our Sun is probably moving towards a point in the constellation of Hercules with an annual velocity of about 1·6 radii of the Earth's terrestrial orbit. A motion so small compared with the velocity of light could not be detected, and the usual theory of aberration might practically be adopted as true were we sure of no other motion. But what we must consider is the *absolute* motion of our Sun in space—that is to say, the absolute motion of translation as produced by the Sun's motion in our own cluster, and the motion of our cluster in space *.

* The importance of the settlement of this question is probably even greater in its physical than in its astronomical aspect. Should a real difference be found in the constant of aberration as determined by different stars, and a reasonably satisfactory agreement of the various results be obtained on the supposition that the differences are caused by the absolute translation of the Earth through space, then two important conclusions result:—

(*a*) We obtain some idea of the direction and amount of motion of our nebula (the Milky Way) in space.

(*b*) We may reasonably conclude that our theory of light is correct, which supposes that a ray of light is transmitted through space with uniform velocity independently of the velocity of the source of light, and that the ether is fixed and infinite—that is, is nowhere limited in extent and is not connected with our Solar System, or even the system in which our Sun is a star.

On the other hand a negative result would go far to show that our conception of ether is not correct; at least would force us to adopt one of two conclusions:—either that the Milky Way is nearly stationary in space, or that the ether accompanies our Solar System or the Milky Way and is not fixed in space and not infinite.

The practical settlement of the question might be satisfactorily attempted in the following manner:—

A skilled observer should establish a Prime-vertical Transit near the Cape

Digitized by Google

To obtain the solar parallax with the accuracy required, the constant of aberration must be determined with an accuracy of $\frac{20''\cdot445}{1000}$, say $\pm 0''\cdot02$; and this presumes that the light-velocity determination has no sensible error. If the conclusions of Argelander, Struve, and Peters, as to the motion of the solar system in space, are correct, the maximum effect of this motion on the constant of aberration is only $0''\cdot0005$.

But who can say that these conclusions are as yet definitive? The question must remain unsettled until a large accumulation of exact spectroscopic determinations of the apparent motions of stars in the line of sight have been made, and the results, as tending to show the motion of the Sun amongst the stars, discussed and compared with the same results derived from the apparent motions at right angles to the line of sight. Still less can any one at present say that the Milky Way (of which our Sun is a component star) is stationary in space. It is hardly possible to conceive that it can be so: and, if in motion, how do we know that its annual motion does not amount to 36 diameters of the Earth's orbit? Such a motion is utterly insignificant compared with the immensity of the Milky Way itself, inconceivably minute compared with the vast orbit which the Milky Way must describe; yet such a motion will alter the value of the constant of aberration $\pm 0''\cdot02$ for certain stars, and would therefore require a special investigation to make the result have the accuracy which we seek.

The author of this article remarks as a noteworthy fact that small as are the differences in the values of the aberration deduced by Struve from the various stars, they are still considerably greater than they should be according to the values of the individual probable errors; and if we arrange the results, as in our Table, in the order of Right Ascension (for some reason the results are never published in this order), we find a remarkable sequence, as if the absolute motion of the solar system was really measurable and the Earth's axis somewhat inclined to the direction of motion. The material is of course totally inadequate to the solution of the problem, or even to supporting speculation on the subject; and we mention it only to show that Struve's measures are not opposed to the possibility in question. Apart entirely from this it is evident,

---

of Good Hope or near Sydney, and make a series of observations (similar to those made by Struve) for a period of about 18 months. He should then proceed to Algiers and make a similar series of observations during a period of $2\frac{1}{2}$ years, and finally return for a year and a half and reobserve the stars at the station in the southern hemisphere. The observations would not be at all of a trying or laborious nature, and a very powerful and independent check might be obtained by employing in addition observations of the difference of zenith-distance of pairs of stars having nearly the same R.A. and nearly equal and opposite zenith-distance by means of Talcott's zenith telescope. Considering the great importance of the question, six years of an astronomer's life might well be bestowed upon it.

Digitized by Google

from a careful consideration of M. Villarceau's reasoning *, that no conclusion can be drawn as to the definitive value of the aberration, or any definitive result for the solar parallax be founded upon it until some such investigation as that indicated by him has been executed.

In *résumé* of what has been said we have arrived at the following conclusions:—

A. It is unsafe, in the present state of science, to adopt any *definitive* conclusions as to the solar parallax founded on theoretical considerations, and it will *always* be unsound to do so if theory cannot be checked by sufficiently exact observation.

B. That, having regard to the high accuracy of M. Le Verrier's investigations and the grounds given to show that his results are affected with very small uncertainty,

(1) *No method of instrumental determination which does not promise an accuracy of* 1-1000*th part of the whole parallax should be employed.*

C. To all instrumental methods two further tests (besides the preceding) must be applied.

(2) *No method can be admitted which in the remotest degree is liable to systematic error; and every method which is not "self-correcting" is liable to such error*: that is to say, *the observations themselves must afford the material for the determination and elimination of all the errors to which the method can be shown to be liable.*

(3) *The method must rest on a simple system of rigid accuracy, both as to observation and reduction, so that every theorist or computer must, from the same series of observations, arrive at precisely the same result.*

D. The physical method, though affording an interesting confirmation of the other methods, cannot be considered ripe for employment until new Tables of Jupiter's satellites, and a new determination of the constant of aberration, as indicated by M. Villarceau, have been completed.

In continuing this paper we shall proceed to apply these three tests to the various instrumental methods which have been proposed or employed to determine the solar parallax, and endeavour thus to arrive at some conclusions as to their various merits.

DAVID GILL.

The Scintillation or Twinkling of the Stars†.—Part I.

THE scintillation of the stars is a phenomenon which is not only full of interest for those who are acquainted with the merest outlines of

* The reader should consult M. Villarceau's paper in the 'Additions' to the 'Connaissance des Temps' for 1878.

† The substance of a Lecture recently delivered at Gresham College.

Digitized by Google

time before the observation; nevertheless the light has suffered its physical aberration to the full extent implied by the law of nature, the ultimate effects being modified by the combined motions of the Earth and Moon.

C. PRITCHARD.

The Determination of the Solar Parallax.—Part III.

RETURNING now to the instrumental method, we have to consider:—

1. How are the relative position and distance of the two observers determined?
2. How are the two observers to make their observations simultaneously?
3. What are the most suitable objects for observation? and
4. What means are to be employed to determine the difference of *direction* of the object as viewed by the two observers?

A complete answer to these questions is the solution of our problem.

1. The Earth's radius at the two points of observation, the latitudes of the stations of two observers, the hour-angles and declination of the object to be observed being known, we have the data for computing the relation of the parallax at the instant to the Planet's equatorial horizontal parallax.

The results of surveys of various portions of the Earth's surface have given, with very considerable accuracy, the radius of curvature of the Earth in the neighbourhood of these surveys; and combinations of these, on the supposition that the Earth is a spheroid of revolution, have yielded values of the Earth's mean equatorial and polar diameters, which are undoubtedly very close approximations. The recent completion of larger and more exact surveys has shown that the Earth is by no means a spheroid of revolution; and attempts have been made by Clarke and others to find the ellipticity of the equator. The material, however, at the theorist's disposal is not yet sufficiently complete.

Thus, for example, the radius of the Earth near Central Europe or near Central India is known with very considerable precision, and with somewhat less, though still considerable, accuracy near the Cape of Good Hope.

The results of the great geodetic operations of the United-States Coast Survey now in progress, and the triangulation of Java just completed by Dr. Oudemans, will afford valuable data; if to these could be added the results of accurate and extensive arcs of latitude and longitude in Australia and South America, much would be done towards the solution of the problem. Another valuable contribution to geodesy would be the determination of an arc of meridian along the Nile. The river runs north and south, with mountains on either side, during a great part of its course. Boats afford an easy means of transport, and the hills on either side provide excellent

Digitized by Google

stations for trigonometrical points. The writer of this paper has already measured a base-line near Cairo, suitable for the commencement of such an operation; and it is to be hoped that the day is not far distant when the operations will be carried out. Finally, the completion of a geodetic survey of New Zealand would complete the data for determining the true form of the Earth. Meanwhile we must be willing to admit an uncertainty of at least half a mile, and possibly an error of a whole mile, in the length of any particular radius near the equator. An error of a whole mile, however, only implies an error of $\frac{1}{4000}$ part of the Earth's radius, and therefore only an error of $\frac{1}{4000}$ in the resulting parallax. For the accuracy which we can at present look to attain in the determination of the parallax, we may therefore consider the form and dimensions of the Earth sufficiently well known. Indeed, should the time arrive when our methods of observation have been so improved that an instrumental determination of the solar parallax more accurate than $\frac{1}{1000}$ of the whole becomes possible, then may we look to obtain a more sure and speedy determination of the form of the Earth's equator by purely astronomical methods than by the operations and calculations of geodesy. Suppose that some means of observing the angular distance between the Moon and a star were discovered so accurate that we might be sure of the determination to $\frac{1}{10}''$; then it would be possible to determine the place of the Moon with a probable error of $\frac{1}{10}''$ of arc; and this, if done in the evening and early morning at an equatorial station, would determine the lunar parallax with a probable error of less than $\frac{1}{10}''$. Certainly, by continuing a series of such observations, this degree of accuracy could be attained in the final value of the constant of the lunar parallax. If such a series of observations were made at a number of different points round the Earth's equator, the different values of the constant of lunar parallax would be proportional to the Earth's radius at the various points of observation. An error of $\frac{1}{10}''$ in the constant of the lunar parallax would correspond to an error of less than $\pm$ 0·12 mile in the Earth's radius, a degree of accuracy sufficient even for the discussion of the third decimal of the Sun's parallax. Perhaps it is doubtful whether such a degree of accuracy will ever be attained; but, at the same time, the writer believes that by observing a star and a minute well-defined lunar spot, by means of the heliometer, a closer approximation can be attained than is generally supposed possible.

The determination of local time, of the latitude, and the hour-angle, with all necessary accuracy, present no difficulties whatever; and we may with safety consider that the result will not sensibly be affected by any errors in these determinations. We pass, therefore, to

2. How are the two observers to make the observations quite simultaneously?

That the observations should be actually made quite simultaneously is of course almost impossible, nor is it necessary, so long as the movement of the planet observed is known. What is necessary, then, is merely that the *absolute instant* of each observation should

Digitized by Google

be known; that is, the time of the observation as determined at some standard meridian, such as Greenwich.

Thus if one observer note the *direction* at $4^h\ 0^m$ Greenwich mean time, and another at $4^h\ 30^m$ Greenwich mean time, we might very properly reduce both observations to the common epoch $4^h\ 15^m$ by applying the correction for tabular motion of the planet during $+15^m$ to the first observation and during -15^m to the second.

Now both observations would be just the same as if they had been observed at the same instant, and the resulting difference of direction will be due entirely to parallax, provided there is

1. No error in the observation of Direction.
2. No error in the Greenwich Time.
3. No error in the Tabular Motion.

The consideration of the errors which affect the observation of direction can only be discussed when we come to treat of the various modes in which *direction* may be determined.

The determination of the Greenwich time of observation implies exact knowledge of the local time and the longitude. The determination of local time is a very easy matter, and we shall not stop to discuss it here; but the determination of the longitude presents very serious difficulties, when none of the most direct and accurate methods are available.

If two stations are so close to each other that some signal, such as the bursting of a rocket, can be seen from both, then if the local time of bursting is recorded at both stations, the difference of the local times so recorded is the difference of longitude. Or, again, if a telegraphic signal is perfectly instantaneous, a signal sent at a special instant by one observer at one station would be recorded at the same instant by another observer at a distant station; the difference of the local times so recorded is the difference of longitude.

If the current takes any sensible time to travel from one station to the other, then if station A sends to station B, the clock B will appear to be faster on A than it should be, because of the delay in the transmission of the signal. But, now, if station B sends to station A, the clock B will appear to be slower on A than it should be, for the same cause. If, then, we can suppose the time of transmission the same in the two directions, the mean of the two results will be free from error due to time of transmission. Practically it is found that in well-arranged operations of this kind the results given by different routes and different observers (when due account of the personality of the observers is taken) are in perfectly satisfactory accord. For example, the results for the difference of longitude, Bombay — Madras, determined in 1875–76 by two different routes, differed by only $0^s{\cdot}035$.

In a Transit-of-Venus expedition it is, however, quite impossible to find stations, otherwise suitable, which have all been connected by telegraph with standard observatories, and recourse must be had to other methods.

Some signal to represent our rocket must be found in the heavens,

Digitized by Google

a signal visible over a large portion of the earth. Such, for example, would be an eclipse of one of Jupiter's satellites; and if the phenomenon were sufficiently precise to be observed with great accuracy it would afford an excellent means of determining the longitude of all stations from which the eclipse in question was visible. But the phenomena are not of that precise character, and have not hitherto been found to give results of the required accuracy.

The instant of bursting of a meteor certainly presents a suitable and sufficiently well-marked celestial phenomenon; but the appearance of well marked meteors is of too rare and uncertain occurrence, and the circle within which each is visible (200 to 400 miles diameter) is too limited to admit the method as one of general use for longitude determinations.

In this case recourse must be had to the Moon. The Moon's motion being comparatively rapid, we have only to find the local time at which it arrives at a certain position, then if we know the Greenwich time at which it should reach the same position, the difference of the two times is the difference of longitude. The accuracy of a longitude so obtained will depend on the accuracy with which the Moon's place can be predicted and the accuracy with which it can be observed.

The existing tables of the Moon are undoubtedly imperfect; but there are now so many observatories at which the Moon is observed that it becomes possible by a simple graphical construction representing the discordance between the observed and the tabular place, to find the corrections of the tabular place with very considerable precision (see Capt. Tupman's paper, 'Monthly Notices' for March 1877, vol. xxvii. p. 249). Various theorists are also at work endeavouring to improve the lunar theory; so that we may hope in some time not very far distant to have tables representing the Moon's motion more accurately than we can find it from the observations of one lunation.

Either by one course or the other we arrive at very considerable precision in obtaining the error of the tables of the standard observatory; the next question is, How is the Moon's place to be determined at the Transit-of-Venus station? The method of Moon culminations is the simplest of all; that is to say, we observe with the transit-instrument the R.A. of the Moon's limb at the instant of transit over the meridian of the place of observation. We find the Greenwich time at which the Moon has the same R.A.; the difference between that and the local time of the observatory is the difference of longitude.

This observation is not affected by the error of the Moon's parallax, is very easily made, and has always been a favourite method in practice. It is subject, however, to a curious systematic error, caused by the fact that very few observers have the same habit of observing the Moon's limb; that is to say, if the Moon had no proper motion, but was absolutely fixed in the heavens, different observers would differ more than $\frac{1}{10}$ of a second of time as to its R.A. Since the Moon's apparent motion among the stars is twenty-nine times

Digitized by Google

slower than the apparent diurnal motion of the stars, the error of the estimated R.A. of the Moon is magnified twenty-nine times in the resulting longitude. Thus an error of $\frac{1}{10}$ of a second in the Moon's observed R.A. produces an error of $2^s \cdot 9$ in the resulting longitude. It is also found that there are systematic differences in the habit of observing the first and second limb of the Moon, so that different observers get systematically different longitudes from the first and second limb.

Thus, unless a number of different observers are employed, or unless the *personality* of an observer is obtained by a series of observations at a standard observatory before and after the longitude determination, the resulting longitude may be three or four seconds in error, though its "probable error," as deduced from the agreement of the observations *inter se*, may be but a fraction of a second. To determine the *personality* of an observer it would be only necessary for him to erect his transit at a standard observatory and make a series of meridian observations. Then from the meridian observations at all the standard observatories deduce the corrections of the Luuar tables and the longitude of the place of observation.

The difference between the longitudes so deduced and the known longitude of the standard observatory would be the result of this personal error, which should be taken separately for each limb, and be applied to all the longitudes deduced by the same observer.

The following Table, extracted from Dr. Gould's account of the Transatlantic Longitude, proves the reality of the facts stated, and shows the very great uncertainty of longitudes determined when reliance is placed in the results of Moon culminations alone.

	Washington W. of Greenwich. h	m	s
Reductions by Walker from Cambridge, U.S., observations, 1843–45	5	8	10·01
„ Loomis from Hudson observations, 1838–44			9·3
„ Gilliss from Capitol Hill observations, 1838–42			10·04
„ Walker from Washington observations, 1845			9·60
„ Newcomb from Washington observations, 1846–60			11·6
„ Newcomb from Washington observations, 1862–63			9·8
The longitude found by telegraph was	5	8	12·39

It must be remembered that no expenditure of care or trouble was spared, either in the observation or reduction, to obtain these results. The instruments were the best to be obtained, and were surrounded with all the appliances and comforts of first class observatories, circumstances which cannot be attained easily in connexion with an expedition.

It is probably yet to be shown how far the personality of an ave-

Digitized by Google

rage observer in observing the Moon may be considered constant; and it would be of great and practical interest to see such a series of observations carried out at two sufficiently separated intervals. It has been suggested that instead of observing the Moon's limb a well-defined spot on the Moon should be observed.

Such a series of observations has been made in connexion with the United-States Coast Survey; but the results were not quite so satisfactory as had been expected.

The occultation of a star by the Moon presents a phenomenon easy of accurate observation; and hitherto this has afforded the most accurate results for longitude by the Moon.

The *personality* of the observer is reduced to the small fraction of a second, which is all that affects different observers in noting a sudden phenomenon; but there are still sources of error which are important. The irregularity of the Moon's disk, the error of the Moon's mean parallax, the uncertainty of the radius of the earth as affecting that parallax, are all sources of accidental error—accidental because they can be eliminated by the same observer. Thus the observation of numerous stars at different parts of the limb may be considered to eliminate the irregularities of the limb; and occultations occurring east and west of the meridian may be considered to eliminate errors in the parallax, as also do occultations observed near the upper and lower limb not far from the meridian, when the Moon has considerable zenith-distance.

There still remain such errors as are caused by the difference in the apparent diameter as affecting an occultation when the occultation takes place at a dark or illuminated limb. Thus it is easy to suppose that an occultation will take place sooner when the limb is bright than when it is dark, and that the contrary will be the case in reappearance. If these amounts were precisely alike, then by equally observing disappearances and reappearances equally before and after Full Moon, we should have a nearly perfect longitude determination. Practically Dr. Auwers at Thebes found this to be the case; the result for his longitude, as deduced by the Moon, agreed with that deduced from telegraphic determinations within the limit of probable error. To complete the accuracy of the occultation method it seems to the writer to demand a still further development, which is this—that the corrections of the Moon's tables be arrived at by precisely the same method as that by which the observations for longitude are made. For some days after New Moon and up to First Quarter, especially in tropical latitudes, it is possible in a very short time to accumulate a very considerable number of observations of occultations at the dark limb. The absence of twilight enables the observer to note occultations of stars down to the 8th and 9th magnitudes, and the *lumière cendrée* renders the dark body of the Moon sufficiently visible to obviate the necessity for any prediction of the time of occultation.

The observer has therefore only to search for a star near the dark limb of the Moon, set the wire of his position-micrometer so as to

Digitized by Google

cut off a small segment of the Moon, and note the instant of occultation. The reading of the position-circle gives the position-angle at the point of disappearance with all necessary accuracy for after-identification of the star. The star-places can afterwards be determined with any desired precision by meridian observations at different observatories, and the equations resulting from all the occultations will give the corrections to the Moon's tables and the longitudes of all the stations if the longitude of one or more of the stations has been previously accurately determined by independent means. Such a method will not be quite perfect as a means of determining the place of the Moon's centre, because the resulting corrections of the tables would be derived from one limb only; but the resulting corrections will be right for that limb, and the resulting longitudes will also be correct. Should the next Transit of Venus be observed, it seems very desirable that this method should be employed for the longitudes of the stations, and the place of the Moon be determined by occultations observed at places of known longitude.

Reference has already been made to the possibility of determining very accurately the place of the Moon by means of heliometer-measures of the distance and position-angle of known lunar spots from stars. Should this method be found to answer in practice, the results would of course afford the longitude of the station, and probably with very great accuracy if similar observations were made at two stations, one of them being a standard observatory. It would be, in fact, simply a refinement of the old well-known method of lunar distances.

Besides the telegraphic and lunar methods a third method exists for determining longitudes which has been used with great advantage, viz. the chronometric method. If a chronometer is perfect (that is, if it goes accurately to time, or has quite a uniform rate), then we have simply to find its error at a standard station, transport it to the station whose longitude is required; the difference between its predicted error and the error found by observation is the difference of longitude. No chronometer, however, is perfect, and it has, besides the imperfections due to change in oil, imperfections of construction, &c., other errors which follow a law depending on the temperature. The question of the errors to which thermometers are subject, the causes of these errors, the methods for their correction, and the best *modus operandi* generally in chronometric expeditions is too long a subject to be entered upon here; and we shall for the present content ourselves by stating some general conclusions derived from experience as to the value of the method.

1. No new chronometer is of much value for refined purposes; and, if possible, a chronometer should be sprung for six years before it is employed for scientific work.
2. Each chronometer should be cleaned about three months before being employed.
3. The rate of the chronometer should be obtained in three tem-

Digitized by Google

peratures at least; these temperatures should include the extremes to which the chronometer is likely to be exposed. The chronometer should remain at least a week in each temperature, and the tests should be symmetrical as to temperature; that is, if the temperatures are *a*, *b*, *c*, the tests should be *a*, *b*, *c*, *c*, *b*, *a*.

4. After the voyages the same tests should be repeated.
5. From the results of the tests, tables should be constructed to give the temperature corrections, according to a law which assumes the rate to vary with the temperature and the square of the temperature, and which can be expressed otherwise by saying there is a temperature at which every chronometer gains more than at any other temperature, that this temperature has to be determined for each chronometer. The rate of the chronometer varies as the square of the temperature from this point; and in most chronometers this variation is nearly expressed in seconds by the square of the number of degrees Fahrenheit from this point, multiplied by the constant ·003.
6. The absolute rate of the chronometer must be regarded as unknown and determined (after the temperature corrections have been applied) from the rates before and after the voyage, or, better still, by the rates so derived from a voyage starting from and returning to the same point.
7. A chronometric thermometer (that is, an uncompensated chronometer) must be employed to determine the temperatures.

Most chronometers are adjusted for a maximum gaining point of 65° to 70°, most generally the latter.

The actual corrections for temperature of an average chronometer, whose maximum gaining point was 68°, would be as follows:—

Temp. (Fahr.)	55°	60°	65°	70°	75°	80°	85°	90°	95°
Rate (losing)	0ˢ·54	0ˢ·20	0ˢ·03	0ˢ·01	0ˢ·16	0ˢ·46	0ˢ·92	1ˢ·54	2ˢ·32

That is to say, a chronometer, which in the ordinary temperature of the Mediterranean would be going almost perfectly, would on entering the Suez Canal and the Red Sea lose two seconds a day. This error might not be so great in all the chronometers, but it would be greater in others, and, on the mean of a number of average chronometers, would undoubtedly be more than one second daily. When chronometers are employed in operations in which they are subject to great changes of temperature, some such correction is absolutely essential; and in such circumstances no longitude determined without some such correction can be considered of value. But if the precautions described have been taken, experience has shown that ten such chronometers, for a voyage of fifteen days, will give a longitude more reliable than by any method except the telegraphic.

That means do exist, therefore, for the determination of the longitude of any station to one second of time is sufficiently evident; but

Digitized by Google

it is not yet possible to say how far all the various expeditions in the last transit have availed themselves of these means. For the present, we shall suppose they have done so, and assume that the longitudes of the various stations are known to one second of time. If, however, the same observer avails himself of the Earth's rotation to transport him to the different stand-points from which to view the object whose parallax he desires to measure, the difficulties and labour connected with the longitude operations disappear, as all the observer requires is a knowledge of the error and rate of his clock on local time, and an approximate knowledge of his longitude.

He can thus allow for the motion of planet in the interval between his evening and morning observations; and the difference of the resulting positions is the effect of parallax. We can, therefore, in considering the various instrumental methods, divide them generally into two heads:—

1. The methods involving at least two observers.
2. The methods which may be performed by one observer, or the method by the diurnal parallax.

DAVID GILL.

---

*The Scintillation or Twinkling of the Stars**.—Part II.

THE researches of M. Respighi, Director of the Observatory of the Capitol at Rome, published by him in notes to the Accademia Pontifica de' Nuovi Lincei in 1868 and 1869, and also to the French Association for the Advancement of Science at Bordeaux in 1872, show some very interesting results obtained by means of the spectroscope. When the light of a star is spread out into a spectrum and the star is scintillating, certain bands or stripes, sometimes dark, sometimes luminous in character, are seen to traverse the spectrum from red to violet, or *vice versâ*, or sometimes to oscillate between the red and the violet. When the star is near to the horizon these bands are perpendicular to the length of the spectrum, whether we form that spectrum in a horizontal or in any other direction. If we keep the spectrum horizontal, then, as the altitude of the star increases, these bands become inclined to the vertical (still continuing to pass in succession and parallel to each other across the spectrum) until when the star has an altitude of 30° to 40° they become horizontal, and each band affects at the same moment all the colours of the spectrum throughout its whole length. At a higher elevation they become gradually fainter and less regular. The ordinary black Fraunhofer lines of the spectrum remain unaffected. If the spectrum be formed in other directions than the horizontal, similar effects are seen, the direction of the bands bearing the same relation as before to the direction of the length of the spectrum, quite independently of what this latter direction may be. The *luminous* bands are only well seen when stars are near the horizon. In bad weather

* The substance of a Lecture recently delivered at Gresham College.

Digitized by Google

The Determination of the Solar Parallax.—Part IV.

The opportunities most favourable for determining the parallax are afforded by the planets Venus and Mars, when the former is in conjunction and the latter in opposition. Of these, Venus is the more favourable in its geometrical conditions, as she approaches the Earth within nearly one fourth of the Sun's mean distance, whilst Mars, in the most favourable circumstances, approaches only within one third of the same distance.

But we have also to consider how the *direction* of the planet is to be measured on these occasions. When Venus is near conjunction she is only visible as a slender crescent in the neighbourhood of the Sun, and at conjunction is only visible on the occasion of a transit across the Sun's disk. It generally happens, therefore, that the only means of determining the *direction* of Venus near conjunction is to refer the direction to that of the Sun's centre. But the Sun's place is also affected by parallax, so that when the position of Venus is referred to the Sun the parallactic displacement is only the difference of the parallax of the Sun and Venus. Mars, on the other hand, can be referred to stars of which the parallax is absolutely insensible; thus it happens that the advantage of Venus in point of parallactic displacement is diminished till the geometrical conditions are only 5 per cent. in favour of Venus.

There is, however, this great advantage in the method of the Transit of Venus, that while the Sun's centre is employed as the point of reference, the Sun's limb may be used as the means of determining the direction. Thus if an observer at each of two widely separated stations observes the absolute instant of apparent internal contact of Venus with the Sun's limb, he is sure that the centres of the Sun and Venus are separated by an angular distance equal to the semidiameter of the Sun, minus the semidiameter of Venus. The difference of the absolute times at the two stations is due to parallactic displacement, and the planet's tabular motion being accurately known the amount of displacement becomes known. If instead of one contact only, the two observers note the instants of internal contact both at ingress and egress, then they practically find the chords described by the planet as seen from both stations. The difference of these chords (in time) being known, as well as the approximate diameters of the Sun and Venus and the tabular motion, we have the data for computing the difference of least distance of centres at the two stations; and this difference being due to parallax, we have the means for computing the parallax. This latter method (originally proposed by Halley) has the advantage of not requiring a rigid determination of the *absolute instant* of each contact, but merely of the duration of the Transit; in other words, does not require a rigid determination of the longitude.

It was Halley's opinion that the instants of contact could be noted with an accuracy of two or three tenths of a second of time; and experiments on models, imitating the contacts of Venus with the Sun's limb, show that the phenomena of internal contact can be

Digitized by Google

noted with an accuracy not very far short of this amount. But experience, in the case of the Transits of the last century, as well as of the recent Transit of 1874, shows that such a degree of accuracy cannot by any means be attained in observing the real phenomena of contact. The reports of the recent Transit are not yet officially published; and therefore the material at disposal for a complete discussion of the subject is hardly yet available.

We know enough, however, to assign the leading causes of the difficulty of observing the true contact; they are chiefly:—

1. Irradiation and diffraction.
2. Disturbance of the image by irregular refraction in the Earth's atmosphere.
3.—The effect of the atmosphere of Venus in distorting the limbs at the point of contact.

Of these, only the first has in general been studied in the artificial models; but the experiments have been very complete, especially those which have been recently conducted at the Paris Observatory. The facts there developed are of exceeding interest, and in some degree explain the discordant appearances described by the different observers. They show, for example, that the appearance or non-appearance of the black drop depends on the aperture of the telescope, the brilliancy of the image, the depth of shade employed, and, of course, also the perfection of the focal adjustment. More recently Professor Newcomb has published a paper ('Monthly Notices,' vol. xxxvii., p. 237) giving the results of experiments with a model placed so far from the observer that the disturbance of the image produced by the Earth's atmosphere is, at least to some extent, imitated. These experiments extend our knowledge of the causes of discordance amongst various observers, but still do not account for the large discordances of actual experience.

We must look for the chief cause of the great discordances, found by experience of the last Transit, in the atmosphere which surrounds Venus, producing phenomena in the actual contact which have not the precise well-defined character presented by those of the model. In the records of the last Transit, to which the writer has had access, are found sometimes a series of observations recorded by two observers at the same station, faithfully describing a succession of phenomena in nearly the same language, but by no means at corresponding instants. It seems as if the observers have been long trained in observing an artificial model, and they naturally expect to find in the real Transit the phenomena they observed in the artificial Transit. They naturally, therefore, look out for certain phenomena, and when they believe these phenomena present themselves they record the time and describe them in language which they had previously found to convey the description of the phenomena they had *intended* to observe. Another observer, less influenced by previous impressions, endeavours to find some phenomenon of a well-marked character, something that he can record to a second, but he finds nothing of the kind. He sees between the limbs a grey colour becoming gradually

Digitized by Google

white (if he observes the internal contact at ingress), or a grey colour becoming gradually black (if he observes the internal contact at egress), and then,—then he notes *something*, he finds it difficult to say what, but describes it in the best way he can.

In fact, in most cases, the first two causes of disturbance are in general exaggerated by more or less imperfect focusing of the observer's telescope, for at all times focusing the telescope on the Sun is a most delicate and difficult matter.

To make the experiment properly, it is necessary to have a focusing scale attached to the sliding tube; thus provided, the most experienced observer, who has not before tried the experiment, will be astonished at the wide differences he will find between successive attempts to focus on the Sun's limb. He will be still more astonished if he will direct the telescope to a difficult double star, and set the focal point successively to the readings he found in his attempts to focus on the Sun. He will find that *with many of the readings the double star cannot be seen as a double star at all.*

Yet there is no doubt that many of the Transit-of-Venus observers took no greater precautions in focusing than to get the Sun's limb apparently sharp; and it is probable that many observations were so made when the telescope was not in sufficiently good adjustment to show the duplicity of a double star of $1\frac{1}{2}''$ distance, much less to exhibit the last delicate contact of two disks.

The cruel manner in which all the phenomena are modified by the atmosphere of Venus is not easy to express; but that the general effect has been to prolong and render indefinite the phenomena of contact, there is no doubt.

In many cases also, the illuminated atmosphere of Venus, surrounding the planet has been confounded with the light of the Sun, and has thus caused very considerable errors of observation. Altogether, in the discussion of observations of contact, we find ourselves placed in very great difficulty. We find at one station one series of phenomena described in language nearly identical; at the corresponding opposite station we find an entirely different series of phenomena described, and it is impossible satisfactorily to identify the corresponding phenomena at the two stations. Indeed *the final value of the parallax at the two stations will entirely depend on the interpretation put upon the language of the observers.* Even if the theorist does succeed in putting the true meaning on each observer's words, he would then probably find that really different phenomena were observed at the opposite stations, and that some assumption is necessary to render the observations comparable.

On the whole, both from what we know of the uncertainties of the observations of contact, and also of the results of the actual combination of various stations for parallax, we are driven to the conclusion that the method of determining the solar parallax by eye observation of contact at a Transit of Venus has been a failure, nor can the method be lookod upon as a means of affording a final and satisfactory solution of the problem. It fails in so far that its final

Digitized by Google

result depends more or less on assumptions; and so long as any assumption is made we have already shown that that method must be rejected.

But we are not by any means confined to the method of contact for determining the place of Venus relatively to the Sun's centre. The labours of De La Rue, Rutherfurd, Draper, and others have shown that photography is capable of giving results of high precision. It is obvious that if at two sufficiently separated stations photographs can be obtained in which the limbs of the Sun and Venus are sufficiently well defined, the distance of the centres of the Sun and planet can be obtained with extreme accuracy by after measurement, provided only that the pictures are affected by no systematic errors. That this latter condition may be fulfilled, the following appear to be the essentials:—

1. The picture must be formed on the photographic plate without distortion, or if affected by distortion, that distortion must be ascertained and allowed for.
2. After the image is formed we must be certain that no change takes place in developing and fixing the picture, or at least we must have the means of detecting such change if it takes place.
3. The angular value of one inch on the plate must be accurately known, so as to convert measured distances into arc.

In considering the requisite process we find at once that it is necessary to employ an image of considerable size, because otherwise the particles of collodion, if magnified so much as to permit measurements of the required accuracy, give an irregularity to the limbs quite fatal to accurate work. To obtain an image of sufficient size it is necessary either to employ a telescope of considerable focal length, or to introduce a secondary lens to magnify the image formed in the primary focus. Hence we have two distinct types of photographic apparatus:—

1. Those in which the image is formed by an object glass of long focus.
2. Those in which a telescope of moderate dimensions is employed, and a secondary magnifier is employed to give an image of the desired size.

The first of these methods has been employed by the French, the Americans, and in Lord Lindsay's expedition; the second by the British, German, Russian, and Dutch expeditions.

The use of the object glass of long focus renders mounting of the lens in the usual manner, though not a practical impossibility, yet at least a matter of extreme inconvenience. To overcome this difficulty in all the expeditions where lenses of long focus were employed the telescope was mounted in a horizontal position, and the Sun's rays were reflected by a plain mirror in the direction of the tube's length. It is not easy to conceive that any sensible distortion in the image can be produced by a lens of such long focus even if only of mediocre quality of figure, and, indeed, we are entitled to consider the method

Digitized by Google

as free from any such error; but it is undoubtedly exposed to all the errors of distortion which may be produced by the plane. It is a matter of great practical interest to consider how far it is probable that undetected errors of the plane will affect the result. The Americans have employed for planes curved surfaces of extremely long radius, whilst the French, as well as Lord Lindsay, have employed planes constructed by M. Martin by the method of Foucault. It is not difficult to test the curvature of these planes. Suppose two collimators of considerable aperture adjusted for parallel rays and directed towards each other, so that a speck of dust on the spider-web in the focus of one of them is seen sharply defined at the focus of the other. If, now, the object glass of each telescope is moved through 45° in azimuth, both telescopes towards the same side, and the plane to be tested is so placed that the speck of dust can be viewed by the other telescope by reflexion from the plane, if (the focal adjustment of the viewing telescope remaining the same) the speck is still seen sharply defined, then the mirror must be sensibly without curvature; or if the focus is altered, the change in the focal point affords the means of computing the radius of curvature of the mirror. Or a better plan probably is to adjust one telescope upon the other by reflexion from a large trough of mercury, both being inclined downwards at an angle of 45°. Without touching the telescopes the plane mirror can now be substituted for the mercury, and any change in the focus can be measured. The first of these plans is probably the more convenient when it is desired to test the mirror in the vertical position, the second when it is desired to test it in the horizontal position, and thus it is possible to ascertain whether its curvature is altered by flexure. But such a test alone is not sufficient. It is quite possible that the radius of curvature may be different in different parts, so that without being sufficiently in error to produce a very bad image it may yet give rise to a somewhat different scale-value for different radii of the solar disk.

To examine this it is only necessary to direct a telescope provided with two pairs of double webs in the focus about 30′ apart, and measure the distance of these by means of a heliometer, or by another telescope provided with a wire micrometer, the rays being received from one telescope by the other after reflexion at the mirror under examination. The distance of the fixed webs being so measured in terms of the heliometer-scale or micrometer-screw, the mirror should be turned through 120° and the operation repeated; finally the mirror is turned through another 120°, and the measures again made. If on frequent examination of three diameters in this way the distances of the fixed webs are found the same for each diameter, the mirror may be considered practically perfect. When a mirror will not satisfactorily answer these trials it should not be employed.

Practically it is found that mirrors which are nearly perfect according to Foucault's tests are found perfect under these tests. The effects of minute accidental errors in the planes are of no consequence,

Digitized by Google

they simply make the pictures somewhat less sharp and increase the probable error of their after measurement; but they do not in any way create *systematic* error.

A very much more difficult question to be dealt with is, do these mirrors, when exposed to the heat of the Sun, acquire a change of form? So far as recorded experiments go this has not been found to be the case, but the investigation is attended with some difficulty; probably the annealing of the glass enters into the question as an important factor, and the question must be decided for each mirror employed. It is probable that here we must first look for any discrepancies in the results of the long-focus telescopes, and all photographs taken by the aid of mirrors, in which sensible change of form is produced by exposure to the Sun, must be rejected.

DAVID GILL.

The Visibility of Shooting-Stars.

DURING the last fourteen months, while recording meteor-paths, I have kept an account of the total number visible, and the duration of watch each fine night. The results I have summarized in the following Table, with the calculated horary rate of frequency for the several months. The figures refer to the range of vision of one observer, looking generally eastwards to an altitude of about 40°.

Month. 1876-77.	A.M. observations.		P.M. observations.		Total A.M. and P.M. observations.		Horary number.	
	Duration of watch	No. of Met'rs seen	Duration of watch	No. of Met'rs seen	Duration of watch	No. of Met'rs seen.	A.M.	P.M.
	hours		hours		hours			
1876. April	.....	.....	15¾	89	15¾	89	.....	6·8
May.....	.....	.....	2½	23	2½	23	.....	11·0
July.....	.....	.....	13	94	13	94	.....	8·7
August ...	6	86	13½	103	19½	189	17·2	8·0
September	.....	.....	5¼	26	5¼	26	.....	5·9
October ...	2½	28	20½	127	23	155	13·4	7·4
November	12	133	13¾	79	25¾	212	13·3	6·9
December	6¼	62	14¼	101	20½	163	11·9	8·5
1877. January ...	12	84	6½	31	18½	115	8·4	5·7
February	5¾	53	6	24	11¾	77	11·0	4·8
March	7¾	46	3½	22	11¼	68	7·1	7·5
April	5½	38	2	13	7½	51	8·3	7·8
May.....	4	23	3½	19	7½	42	6·9	6·5
13 months, 1876, April-1877, May.	61¾	553	120	751	181¾	1304	10·74	7·51

Horary number A.M. and P.M. = 8·61.

Digitized by Google

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 5. AUGUST 20. 1877.

The Determination of the Solar Parallax.—Part V.

With regard to errors due to the contraction of the photographic film, the French adopted the daugerreotype method of photography, in which it is impossible to imagine any errors of this kind, as in the collodion process, because the picture is virtually a portion of the silver plate upon which it is taken. But in adopting this process the advantage of measuring the photographs by transmitted light was lost: and it is a practical question, which can only be decided by the result of experience, whether the loss or the gain is the greater.

In the case of the Americans and Lord Lindsay, using the collodion process, it became necessary to provide some means to detect possible contraction of the film after development of the picture This was done by placing the sensitive plate near to, or in contact with, a reticule ruled on glass near the primary focus: this reticule was photographed simultaneously with each photograph of the Sun; hence any change produced during the development would cause a similar change in the relative positions of the images of the ruled lines on the developed plate.

The American arrangement contemplates only the detection of contractions and the rejection of such pictures as show sensible contraction; hence, in their reticules, the reference-lines are comparatively few. In the arrangement of the apparatus used by Lord Lindsay, every photograph is supposed possibly to have undergone distortion by shrinking of the film; the lines of the reticule are only one twentieth of an inch apart, and the measurement is executed by interpolating the position of the limbs of the Sun and Venus, as referred to the two including lines of the reticule. The centres of the Sun and Venus are therefore found in terms of these lines; and the errors of their intervals having all been carefully investigated, it remains only to correct the measures for refraction and convert them into arc, the result being then quite free from error due to the cause under discussion.

Digitized by Google

The long-focus method presents very great facilities for determining the scale-value; it is only necessary to measure the distance of the plate from the posterior surface of the object-glass, and then to determine the distance of the optical centre of the lens from that surface; the sum of these two distances is the radius, of which lines on the surface of the plate (reckoned from the centre of the plate) are tangents.

The determination of the optical centre is not a matter of very considerable difficulty. It is a matter of exact calculation if we know the refractive indices of the flint and crown lenses and their radii of curvature. It is always possible to determine the refractive index with very great accuracy for every ray by grinding faces on the edges of the lenses and then employing them as prisms, and this can be done without materially affecting the effective aperture. We can then attain very approximate values of the curvatures by careful spherometric measures; and having thus found approximately the optical centre, we obtain a close approximation to the true focal length by adding the distance for sharpest focus to the distance of the optical centre thus determined. This determination acts as a check on the curves obtained by the spherometer, and enables us finally to arrive at a determination of the optical centre which is practically perfect. It is then only a question of determining the distance of the posterior surface of the lens from the photographic plate to ascertain the scale-value with any required precision. It is easy enough to measure forty feet with an error not greater than a hundredth of an inch by any of the methods known in the measurement of a geodetic base; and such an error corresponds to an error of $\frac{1}{60}''$ in the Sun's radius, an accuracy which is quite sufficient. So far, then, the method of photography in which no secondary magnifier is employed seems to promise a result of considerable accuracy, unless any hitherto unsuspected causes combine to create systematic error.

The difficulties of dealing with systematic errors when a secondary magnifier is employed are enormously increased, because, first, it is theoretically impossible with the present optical glass (employing spherical curves) to construct a perfect secondary magnifier in which the scale-value shall be absolutely the same in every part of the field; still less is it possible, when the attempt is made, to combine the visual and photographic rays in the same focus; hence every photoheliograph must be separately studied for distortion of the image, and the necessity for this study has always been recognized by the originators of the method. The results of actual trial, however, prove that the total amount of distortion is very much greater than was supposed; and not only so, but this distortion is not the same in each radius. This latter error is probably the result of imperfect centering of the lenses composing the secondary magnifier; and if we actually trace the rays through such a combination of lenses we shall find how very minute is the amount of error of centering which will cause a very sensible elongation of image in one direction.

Digitized by Google

Now let us consider how far the means hitherto adopted are adequate to determine this distortion. It must be remembered that all the systematic errors must be eliminated, because, whatever they are, no number of photographs and no number of measures will eliminate them; and unless in each series of pictures from each instrument Venus has occupied many positions, symmetrically disposed with respect to the Sun and the optical axis of the instrument, the result will only be to give weight to a systematic error common to them all. If the last-mentioned condition could be completely realized, the effect of distortion might perhaps be eliminated, because its effect would have opposite signs. This at first looks plausible; but in reality it is not the fact. During the time of greatest parallax, Venus occupies nearly the same place on the Sun; and, even if we deal with the whole transit, Venus will always occupy one side of the field, limited to about 60°.

It is therefore absolutely necessary to get rid entirely of systematic error, or at least of such systematic error as will produce an error of $\frac{1}{1000}$ part in the resulting parallax. The parallactic displacement of Venus relative to the Sun is about three times the solar parallax; therefore an accuracy of $\frac{1}{3000}$ of the solar parallax would be sufficient, corresponding with $\pm 0''{\cdot}026$ in the measurement of a distance equal to the solar radius; but as the photographs cannot be made on the horizon, and the parallactic displacement is consequently less, we must adopt at least $\frac{1}{50}''$ as the necessary accuracy with which the distortion at any point must be known. The method adopted to determine the distortion has in general been to photograph a number of pieces of metal planed accurately of the same size, and mounted at nearly equal distances apart, a few hundred yards from the photoheliograph: these pieces of metal thus form a species of scale, extending across the greater part of the field; and when the dimensions of the images of these pieces of metal are measured at different parts of the field, their differences of width are supposed to give the means of calculating the scale-value (or the distortion) at different points of the field. If the object was to ascertain the angular value corresponding to a certain small area (as of a sun-spot) on the photograph at different parts of the field, this method would be amply sufficient, and probably would be the most convenient and simple means that could be adopted; but the question before us is of a very different order, and demands an accuracy utterly unattainable by such a method. For example, it is a known fact that with the present photoheliograph when a plate is taken with the Sun's image near the edge of the field, the image becomes very sensibly elliptical. So much is this the case that it has been seriously proposed to employ the *variation* of this ellipticity to detect the position-angle of the major axis of elliptical distortion in some of the instruments in which the cross webs were removed before the investigations for distortion were made. Let us put the case in figures and determine what accuracy will be required in the measurement of the images of the scale to determine the distortion with the

Digitized by Google

required accuracy by such a method. Having no better data to go upon, let us assume that it would not be possible to detect the position-angle of the greatest distortion unless the maximum difference of the diameters for different position-angles amounts at least to $1''$; then it is reasonable to suppose the actual distortion will be at least three times the variation of the distortion, *i. e.* the total distortion will be such as to produce an absolute amount of $3''$ in the difference of the two diameters. Now the true distance of any point on the photographic plate from the centre of the plate, whatever the nature of the distortion (assuming for the moment that the distortion is circular), can be represented by

$$as+bs^2+cs^3+ds^4+\&c.,$$

where a, b, c, are coefficients to be determined, and s is the distance in inches from the centre of the plate. Since the distortion is small in amount we cannot be far wrong in assuming that it will be sufficiently represented by the two first terms of this expression, and that we may call a the true scale-value and b the distortion. If now we suppose the Sun's image to be 4 inches in diameter, the photographic plate 6 inches square, and the difference of $3''$ in the two diameters to occur only when the image of the Sun is taken at the greatest possible distance from the centre of the plate, then if we compute the coefficients of b for the two diameters we shall find

$$\begin{aligned}\text{Distortion of greatest diameter} &= 11{\cdot}4b\\ \text{,,} \quad \text{least} \quad \text{,,} \quad &= 9{\cdot}2b;\end{aligned}$$

hence, since the assumed difference of diameter was $3''$, we shall have

$$\begin{aligned}2{\cdot}2b&=3''\\ b&=0''{\cdot}733;\end{aligned}$$

hence, when the Sun is central, the total distortion of the diameter is $8b=5''{\cdot}85$. Even selecting the most central pictures only we see that the mean distortion of the diameter will exceed $6''$, *i. e.* twice the amount of the greatest difference we can measure. To determine b with the necessary accuracy by this method we must therefore arrive at a determination of the difference of diameters with an accuracy of $\frac{1}{100}''$. If we suppose that each photograph can be measured with an accuracy of $\frac{1}{10}''$ (and this is overstating the accuracy of the best photographs), we shall then require a hundred fine photographs of the Sun, taken under the most favourable circumstances, for our determination, even if we suppose the distortion absolutely circular. This not being so, we have to find some expression for the distortion, such as

$$(as+bs^2)(1+c\sin p+d\cos p+e\sin 2p+f\cos 2p),$$

where p is the position-angle of the line passing through the centre of the plate along which the distance is to be reckoned. Since we require six coefficients to express this distortion, then we shall

Digitized by Google

require six symmetrical sets in the most favourable positions, each containing a hundred photographs, to determine our corrections with required accuracy.

The method appears hopelessly laborious and unsatisfactory, particularly when we consider that to render the investigation entirely rigid it would require to be continued for such variations of the focal adjustment as must occur in actual practice. When an artificial scale is employed in this way the factors are even still more unfavourable, and the case is complicated by the fact that the focal point is very considerably changed; and it is found practically impossible to obtain pictures which admit of measurement, even with an approximation to the accuracy we have mentioned*.

In the ordinary photoheliographs with the secondary magnifier the determination of the scale-value is accompanied with extreme difficulty. In the original scheme it was intended to employ the Sun's diameter as the standard of scale-value, and to interpolate in each picture the distance of centres of Sun and Venus (derived from measures of the limbs) in terms of the Sun's diameter. Experience has shown that the photographic diameter of the Sun is by no means a constant quantity, but increases to a very large extent by exposure. Hence has arisen the proposal to assume that the image of Venus will be diminished by just the same amount as the Sun's image is increased by exposure. If this were so, the sum of the apparent diameters of the Sun and the apparent diameter of Venus will be a constant; and any approximate value of this constant being assumed, the pictures produced by various photoheliographs will be comparable. It is objected to this, and probably with some reason, that the increase of the Sun's diameter will be less than the diminution of the diameter of Venus, because the Sun's image is least bright at the limb, and consequently the intensity of light falling on the plate near the image of Venus would produce a greater amount of so-called "photographic irradiation" than round the limb.

There are as yet no published data of the result of photographic measures upon which to found definite conclusions as to the probable accuracy of the photographic method when a secondary magnifier is employed; but, for the reasons we have given, it seems improbable that in this way the required accuracy can be attained.

DAVID GILL.

[We hope to answer in our next No. some of Mr. Gill's objections to the photoheliographic method by a reference to the actual results for distortion obtained at Greenwich. The question is one of much interest, as well as of some difficulty, and a discussion

* A great improvement would of course be to have the *intervals* as well as the metal pieces at standard distances; the distortion (if central pictures only were used for parallax determination) might be then determined with an augmented factor. The difficulty due to badly defined images would still remain. A great improvement would be to photograph artificial stars produced by heliostats placed at a great distance. This seems to offer the only means of making a really satisfactory determination of the distortion errors.

Digitized by Google

of the whole subject in the light of recent experience will prove of great value in view of the next Transit of Venus. We should be glad to have the views of astronomers who have considered the question in connexion with the expeditions sent out in 1874 by various countries.—EDITOR.]

Photographic Spectra showing the Sun's Rotation.

AT one of the late meetings of the Royal Astronomical Society, I hinted that I had obtained photographic spectra showing the rotation of the Sun by the displacement of the lines; but I did not venture to bring forward the method I had adopted till I had an opportunity of confirming my results. Such an occasion has presented itself to me lately through the kindness of Mr. J. Norman Lockyer. He has placed at my disposal a magnificent diffraction-grating of over 17,000 lines to the inch, ruled on a silver surface, by Rutherfurd. The dispersion in the spectrum of the third order is very great, and has enabled me to verify my method, which I had only tried with a dispersion obtained through three prisms of $62\frac{1}{2}°$. The following is a brief description of the method I adopted in both cases. An 18-inch collimating telescope was employed; and in front of the slit was placed a condenser collecting the rays from a heliostat moved by a clock arrangement, the surface of the mirror being silvered. Between the condenser and the slit was placed a split lens of some 8-inch focal length; and the distances were so adjusted that the image of the Sun on the slit was approximately half an inch in diameter. Both the condensing apparatus and the split lens were corrected for the actinic rays. By a fine screw-adjustment the centres of the half-lenses were separated till the two images of the Sun were tangential to one another, the tangent being at right angles to the length of the slit and halfway up it. After passing through the prisms, or after reflexion from the grating, the rays were received on a lens attached to a camera of some 6-feet focal length, on the focussing-screen of which the spectrum was received. The absolute focus of the spectrum was obtained by trial with photographic plates, as, owing to the portion selected (the H lines) being almost invisible, the plan of using a magnifier traversing a transparent plane could not be employed. It was absolutely necessary with the prisms to select this portion of the spectrum, since the longer wave-lengths theoretically give so much less displacement of the lines. With the grating, since the yellow of one order overlaps the H lines in the next, an opportunity was given to examine visually the effect of the rotation when using a magnifier. The lines all appeared displaced to what I should estimate would be the proper amount when the time of day when viewed was considered. On July 30 with the grating, photographs of the double spectra were taken at intervals during the whole day; and it was interesting to note the displacement, diminishing and again in-

Digitized by Google

Mr. Hill and by himself. In this method the inequalities which are independent of eccentricities and inclination are first determined numerically with all the accuracy desirable; the inequalities depending on the first power of the eccentricities and inclination are next found, and afterwards in succession the inequalities involving powers and products of these quantities, which are of two dimensions, three dimensions, and so on, are similarly determined.

After the first stage of this process all the differential equations which require solution are linear and of the same form. Every coefficient so obtained can be compared at once with the analytical development of the same coefficient in powers of m given by Delaunay. The convergence of the terms when arranged in successive orders with reference to the eccentricities and inclination is so rapid that it is probable that all the sensible corrections of Delaunay's coefficients will be found among the terms of low orders.

The result which the author obtains is the following:—

If m denote as usual the ratio of the Sun's sidereal mean motion to that of the Moon, and g denote the ratio of the mean motion of the Moon from the node to its mean sidereal motion, and if with Plana we take $m=0{\cdot}0748013$, then, in the ideal case considered in this paper,

$$g=1{\cdot}00399,91618,466.$$

The value of the same quantity found from Delaunay's development in powers of m, which is carried as far as m^7, is

$$g=1{\cdot}00399,91723.$$

The author has found that the series which expresses the value of g converges more rapidly when developed in powers of $\frac{m}{1-m}=\mathrm{m}$ suppose. The value of g found from the same number of terms of this series is

$$g=1{\cdot}00399,91591,$$

which is seen to be a much closer approximation than the former.

J. C. Adams.

The Determination of the Solar Parallax.—Part VI. (*Concluded.*)

Besides the methods of contact and of photography employed to determine the relative directions of the centre of Venus and the Sun in a transit of Venus, another method was employed by the Germans, the Russians, and by Lord Lindsay's expedition—viz. the Heliometric method.

By means of the Heliometer it is possible during the whole period of Transit to reproduce *contact* at the will of the observer—that is to say, by displacing the segments of the object-glass the limb of the

Digitized by Google

image of Venus produced by one segment can be made to appear in contact with the image of the limb of the Sun produced by the other segment at the will of the observer, and by setting the segments so that the motion of Venus will cause the limb to approach, a contact can be observed somewhat similar to the true contact. This artificial contact has, however, an important advantage over the contact generally so called, because in this artificial contact we can choose the time when Venus is fully entered upon the Sun and has a uniformly illuminated background; whereas in the ordinary contact, just at the point where we wish a precise observation, there is a violent contrast of backgound. Not only is this so, but the effects of the atmosphere of Venus can be entirely eliminated by the observer making, within a short interval, contacts with the opposite limbs of the planet. Also we can eliminate any personal habit of observation by making the contacts with opposite limbs of the Sun, so that if we can measure the distance of angular separation of the segments of the heliometer object-glass and the position-angle of their line of separation, with extreme precision, we possess a most perfect means of determining the relative positions of the Sun and Venus.

The difficulties in the way of these determinations are, however, exceedingly great. In the first place, the investigation of the errors of the scale or screw by which the amount of separation of the segments is measured is a work demanding exceeding labour, care, and skill, though it is by no means an insuperable difficulty. The determination of the scale-value of the instrument is, however, a work involving very complicated considerations.

At first sight it seems as if the method is free from the necessity for any accurate determination of scale-value, because, if measures are made from opposite limbs of the Sun, the Sun's diameter may be taken as the standard for all observers, and the place of the planet may be interpolated relatively to the opposite limbs.

Unfortunately it happens that there is a very marked difference in observing the Sun's diameter due both to instrument and observer. Thus two observers with different instruments, who have compared scale-value by a number of stars, or zones of stars, will measure Sun diameters with a marked constant of difference.

If the Sun's diameter is assumed to be constant, it, in fact, determines the scale-value; hence the distance of centres measured by the two observers will differ by the proportionate part $\frac{s}{d} \times \Delta d$, where s is the distance of centres, d the true diameter, and Δd the difference of diameter as measured by the two observers. Hence it is only when $s=0$ (that is, when the planet is near the centre of the Sun) that this method can be used.

In the case of the Transit of Venus fully one third of the whole of this personality would enter into the result by this method of reduction. The case is precisely the same as in the photographic method, only that the difference in personality in heliometer observers is very much less than in the difference of diameter in Sun pictures due to

Digitized by Google

difference in focussing and in exposure. For any rigid reduction it is absolutely necessary to have a rigid determination of scale-value.

Unfortunately this scale when determined for any uniform condition of temperature is liable to change, not only according to the temperature, but, especially in Sun observations, from the different temperatures of the different parts of the instrument.

The scale-value depends on the relation of the focal length of the object-glass to the length of the scale; and Drs. Auwers and Winnecke proposed a very complete scheme for determining the scale-value at any instant.

1. The scale-value was determined for a uniform condition of the temperature of the instrument by measuring zones of stars whose places were rigidly determined by meridian observations; and by the same means the temperature coefficient of the instrument was determined for different temperatures, the various parts of the instrument being at a uniform temperature.
2. The effect of a displacement of focus was determined by measuring the Sun's diameter and star distances with different positions of the focal adjustment.
3. The focal point was determined during Sun observations by adjusting the focus on a telescope fixed in a specially prepared chamber, where its temperature would change very slowly, and the temperature of the scale (and hence its length) were measured by a metallic thermometer; hence the proportion of the scale length to the focal length became known.

In Lord Lindsay's expedition similar precautions were employed, excepting that in the latter case the temperature of the tube and object-glass were determined, the former by thermometers, the latter by a thermopile, and the position of the focal point was calculated from these data.

How far all these precautions have been attended with success it is impossible yet to say; the only test is the result of experiment. When the observations of the various heliometer stations have been published and discussed, then we may be able to arrive at some conclusion on the subject.

The measurement of position-angles with the heliometer has always been regarded as possessing an inferior accuracy to the measurement of distances with that instrument. To some extent this must always be so. It implies that the slides which carry the segments shall move strictly parallel to a central bearing. We have to rely on the maker that this bearing shall cause motion in a straight line, and we are always subject to the difficulties which arise from the introduction of oil between surfaces which are limiting or measuring surfaces. Oil has a tendency to arrange itself in layers of varying thickness; and when motion is reversed these layers become no longer of uniform thickness along the bearing surface. The errors so produced enter directly into the measured position-angles with full effect. It is true that in instruments of the highest

Digitized by Google

perfection such errors are very small; yet we can never accept position-angle measures as possessing the accuracy which measures of distance should and do possess. Hence in Auwers and Winnecke's plan for observing the Transit of Venus, the distance was to be measured along, or nearly along, the line of centres of the Sun and Venus, the position-circle readings being only employed to project measures (made approximately in this line) exactly along the line of centres.

The method is that which is most free from systematic error, and as such, when numerous observing stations were employed, was probably the best of all suited for the purpose; but the parallax factors available were, in consequence, much diminished. Thus each observer could only measure the full parallax due to the altitude when the centres of the Sun and Venus were in the same vertical circle. At Mauritius it was only during the early part of the transit that any considerable parallax could be measured; and at every station there was only one instant at which the full parallactic displacement occurred in the line of centres. Oudemans and Pechüle have each proposed methods in which position-angles are employed in order to increase the parallax factor. The method proposed by Oudemans was actually carried out by him in observing the Transit at the island of Reunion, and with what success will be shown by the result. In the experience of the writer, with a first-rate instrument position-angles may be relied on, certainly within 30″, when the method of observation includes the elimination of index error. How far this accuracy can be realized in measures of the Sun in which the index error has to be independently determiued, it is difficult to say. The writer believes that the heliometer measures, in the case of the Transit of Venus, from the exceeding care and great labour bestowed to ascertain and eliminate all sources of error, will give results of considerable accuracy; but at the same time, from the fact that these errors have to be independently determined and applied, and are not eliminated in the course of the observations themselves, they do not possess that entirely satisfactory character so desirable in a delicate investigation.

We must now pass to another method of determining the parallax, which has been carried out with considerable success on different occasions, viz. the method of observing the declination of Mars at observatories in the northern and southern hemisphere. The method consists in observing the declination of the planet by means of the transit-circle, and, to increase the accuracy of the result, the same stars near the planet are observed at the various observatories, so that the method is reduced to measuring the difference of declination between the planet and neighbouring stars.

The effects of periodic error, of graduation of the circles, of flexure of the instruments, and abnormal refraction are thus nearly eliminated, and there remain only the systematic errors which may be supposed to arise from the difference of the habit of the observers in bisecting a star and a planet. The most complete series of observations of this kind was carried out on the occasion of the opposition of 1862, ac-

Digitized by Google

cording to a programme drawn up by Winnecke ; but the most complete discussion of the observations is due to Newcomb (Washington Observations, 1864).

That personality does exist, and that different observers do bisect a star and planet differently, seems to be proved beyond doubt by Newcomb's discussion. That is to say, if we combine separately the results of the different observatories in pairs, we find that the resulting values of the parallax differ considerably more than they should do according to the apparent probable errors ; in other words, there are systematic sources of error which can only be supposed to arise from the different habit of the observer in bisecting a star and planet.

It is not impossible to eliminate such tendency. If the eyepiece of each transit circle were fitted with a small reversing prism, so that the altitude of each star and of the planet could be observed in reversed positions with respect to the vertical, all such personality would be completely eliminated. The method was originally proposed by Kaiser, of Leiden, to eliminate personality in comparing the lengths of standard bars ; but it is capable of very general application in delicate astronomical investigations, and it is to be regretted that the matter was not discussed in time to consider its application in the present opposition.

The observations made in the present opposition will, of course, be valuable to combine with those made in 1862, as probably the observers will be different from those employed on the last occasion ; but without some special provisions to eliminate systematic error no definitive results can be looked for.

The same remarks of course apply when an equatoreal is employed to measure the difference of declination of the planet and neighbouring stars.

More recently Dr. Galle, of Breslau, has proposed a method which appears to offer entire immunity from systematic error, viz. to measure, by means of the equatoreal, the difference of declination between selected stars and a minor planet, or rather to interpolate the declination of a minor planet relative to two stars of comparison. A minor planet presents precisely the appearance of a star, and it is impossible to conceive any personality which can affect the observation of such a planet and star ; and the interpolation of the planet's declination relative to two including star declinations entirely eliminates errors due to error of value of the micrometer-screw. It is true that in the case of the minor planets the angle of parallactic displacement is comparatively small (in favourable cases about $\frac{2}{5}$ of that of Mars in the present opposition) ; but I do not hesitate to say that their minute disks present objects capable of being observed with quite $2\frac{1}{2}$ times the accuracy of observations of Mars. Hence the conditions are equalized. The opposition of Flora in 1874 was observed, at the request of Dr. Galle, by a considerable number of observers both in the northern and southern hemisphere, but in very few cases with the precaution, care, and perfection of instrumental equipment necessary. At the Cape of Good Hope the

Digitized by Google

clamp was unsatisfactory; at Cordoba the micrometer-screw was injured, and so on, so that the results cannot be looked upon as representing the capabilities of the method: and here it is well to remark how very difficult it is to secure, in any plan requiring numerous and widely separated observers, that entire sympathy with the end in view, that scrupulous care in minute detail, which is the first essential of the highest class of observation; and even granting that these have been secured, it does not always happen that distant observatories have, or can be supplied with, the necessary instrumental details in sufficient time. Hence arises the desirability of a method not requiring cooperation, in which success depends on a single observer, who may obtain independently by his own observations a complete series of results.

In 1857 the Astronomer Royal, in an Address to the Royal Astronomical Society on the methods available for determining the solar parallax during the next 25 years, called attention to the present favourable opposition of Mars, and declared his opinion that the best method of finding the Sun's parallax was to determine the difference of R.A. of the planet and neighbouring stars in the evening and early morning by observing transits of stars and planet across the webs of a well-adjusted rigidly mounted equatoreal. So far as the writer knows, no satisfactory series of observations of this nature has yet been carried out, and we cannot therefore form any sound conclusions on the subject; but it would seem that in the hands of a skilful observer the result should be satisfactory *. Of course a very large number of observations would be required, because the observation of a transit is not so exact as the micrometric comparison of two points. It is true in the transit-circle observations the Right-Ascension observations do not fall far short of the accuracy of the observations of the zenith distance; but the latter are mixed up with the whole of the irregular refraction due to the zenith distance of the observation, and thus the error is increased. A promising method, which enables the diurnal parallax to be measured micrometically, is to employ the heliometer to measure the distance between the planet and star. The method is independent of the stability of the instrument, and the measures are only affected by the irregularity of refraction in the short distance between the star and planet.

It is also possible, by selecting stars on opposite sides of the planet, to eliminate entirely errors due to change of scale-value between the evening and morning observations; and if the places of the comparison stars have been determined with considerable precision, the scale-value may be derived from the actual observations, as also may the zero of the position-circle, because errors in either the scale-value or the position-circle zero affect the distances and position-angles measured from opposite stars in opposite ways. By measures conducted in this way, the writer found that in the case of the planet Juno, measured by him in connexion with Lord Lindsay's expedi-

* [Mr. Maxwell Hall has recently applied this method in the island of Jamaica (see p. 263).—Ed.]

Digitized by Google

tion to Mauritius, the distance of the planet could be interpolated between two stars with a probable error considerably less than $\frac{1}{10}$ of a second of arc.

Seeing that, in the observation of a great number of different distances, all different parts of the scale and screw are employed, the systematic effect of division-error is entirely eliminated in a long series of observations. By the use of the small reversing-prism before described, the images of the star and planet in the field of view can be made to occupy precisely the same position relatively to each other and to the vertical in the evening and morning observations; hence any personality here is eliminated: finally any error in tabular motion of the planet can be discovered and eliminated if the series made is a pretty long one, and the places of the stars of comparison are connected by a rigid heliometric triangulation. It is now impossible to conceive any form of systematic error whatever which can attach to such observations; for even if we imagine the constant of refraction to be different on the two sides of the meridian, the effect of this is entirely eliminated when the stars of comparison are situated exactly symmetrically with respect to the planet, or their mean is so. It is also possible to make this error a source of special investigation, or to determine its existence or non-existence by a discussion of the observations themselves.

This method, in fact, entirely fulfils the conditions we have laid down, in the first part of this article, as containing within itself the means of eliminating all possible sources of error. As to its accuracy, if we take the outside limit and assume that the probable error of one interpolation of distance is $\frac{1}{10}$ of a second of arc, and allowing for the impossibility of selecting stars exactly on the parallel with the planet and for the loss of parallax at a mean altitude of observation of about 30°, the available parallax of a well-placed minor planet will average 0·7 of the solar parallax for each evening and morning observation; this would give an error of $\pm\frac{0''\cdot1}{0\cdot7}=\pm0''\cdot14$ as the probable error of the solar parallax derived from each observation. Hence $(14)^2$, or 196 observations, would be required to determine the solar parallax from minor planets with the required accuracy of $\pm0''\cdot01$.

In a good climate we might very readily reckon upon 25 evening and 25 morning observations at each opposition, 50 observations in all; hence *four reasonably favourable oppositions of* favourable *minor planets will give the solar parallax with all the accuracy required by the present state of science,* provided they are observed with a first-rate heliometer by an experienced observer in a good station. The cost and labour of such an undertaking is as nothing compared with the large sums required to equip a Transit-of-Venus expedition, and the results are *capable of only one interpretation.*

The same method of observation can be applied to Mars; and, when a very bright star is near Venus so as to be visible by day, it may be applied to that planet also.

Digitized by Google

The opportunities of observing Mars in the most favourable circumstances are somewhat rare; and though a very large parallactic displacement can be observed, there are still difficulties connected with observing a disk, or rather a disk affected by phase. Though the writer believes these difficulties can be overcome so as to leave no systematic source of error, yet he does not think that the probable accidental error will be so small as in the case of the minor planets; but until the observations now being made by him at the Island of Ascension are reduced, it is not easy to give a sound opinion on this subject. The method of observing the minor planets does, however, alone promise an entirely satisfactory solution of the problem. Its proper execution requires a heliometer capable of dealing with the faintest stars of Argelander's Durchmusterung, an instrument of 6 or 7 inches aperture, and the devotion of 2 or 3 years to the study of its division-errors and constants, and to the execution of the observations. To such a work an earnest astronomer would gladly give so much of his life, and it is to be hoped that ere long such a scheme will be planned and executed.

DAVID GILL.

The Satellites of Mars and the Nebular Hypothesis.

THE rotation-period of Mars is $24^h\ 37^m\ 22^s{\cdot}71$. The inner satellite completes a revolution in $7^h\ 38^m$; but the nebular hypothesis requires that "in every case, unless changed by disturbing forces, the planet should rotate on its axis in less time than is required for any ring or satellite to revolve around the primary"*. In the 'American Journal of Science' for October 1877 the writer called attention to the apparent inconsistency between the facts above stated and the theory of Laplace, and at the same time suggested a *possible* method of reconciliation. The amount of evidence in favour of the nebular hypothesis has been generally regarded as giving it a degree of probability little short of demonstration. Nothing less, therefore, than the development of facts absolutely incompatible with its essential conditions can now be accepted as conclusive evidence against it. Is the motion of the Martial satellites a fact of this description? If so, we have no alternative but to abandon the cosmogony of Herschel and Laplace; if not, the fact itself will probably afford new evidence in its favour.

Effect of Motion through a resisting Medium.—The effect of the motion of any heavenly body through a resisting medium is a gradual approach to the centre of force. We have a well-known instance in Encke's comet, the period of which, even at the present rate of acceleration, will be less than the period of the Sun's rotation

* Mr. David Trowbridge in the Amer. Journ. of Science for January 1865.

Digitized by Google